Letters to the Editor.

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Interspecific Sterility.

THE implications of modern genetics have been so little considered by biologists in this country that the criticism of my address by Dr. Cunningham (NATURE, June 17), though in purpose destructive, is not unwelcome. Of the points he raises one chiefly calls for reply. I directed once more the attention of naturalists to the fact that we still await the production of an indubitably sterile hybrid from completely fertile parents which have arisen under critical observation from a single common origin. So far as our knowledge goes, all the domesticated races—for example, of dogs, of pigeons, of fowls among animals; and of cabbages, of peas, of *Primula sinensis*, and many more among plants—when intercrossed among themselves never produce this sterility in their mongrels, though the races are often distinct enough to pass for species. But if we begin crossing natural species, even those which on our reckoning must be very closely allied, we constantly find either that they will not interbreed, or that, if they can be crossed, the result is more or less sterile. Dr. Cunningham takes exception to my speaking of this interspecific sterility as the chief attribute of species, but he will not dispute that it is *a* chief attribute of species.

The races of fowls might, as he holds, on account of their enormous divergences, be without impropriety compared to natural species. They may also, as he thinks, all descend from *Gallus bankiva* (though I find that difficult to believe); but inasmuch as they do not show interspecific sterility they do not help us to understand how that peculiar property of species arose in evolution. In contemporary variation we witness the origin of many classes of differences, but not this; yet by hypothesis it must again and again have arisen in the course of evolution of species from a common ancestry. The difficulty is no new one; but I emphasised it because naturalists should take it more seriously than they have done hitherto. Especially now that a great deal of experimental breeding is in progress, watch should be kept for such an occurrence. I by no means declare that the event cannot happen, but, so far as I know, it has not been witnessed yet.

Dr. Cunningham tries to fill the gap by adducing two instances. The first is that of *Oenothera gigas*. Now I had not forgotten the tetraploids, which so often do not breed freely with diploids, but the applicability of that example is exceedingly doubtful. Interspecific sterility or incompatibility may well be a consequence of nuclear diversity, though we can scarcely regard an unresolved pair of twins, such as the tetraploid must be, as a specifically distinct organism.

His second illustration, if authentic, would be more nearly what is wanted. He says that "two mutants of Drosophila in Morgan's experiments are almost completely sterile with one another." The allusion is probably to a paper of Metz and Bridges (Proc. Nat. Acad. Sci., 1917, iii. p. 673), in which they claimed to have found two mutants of *D. virilis* which gave partially sterile hybrids when intercrossed. Dr. Cunningham is not perhaps aware that this claim was afterwards withdrawn (Metz, *ibid.*, 1920, vi. p. 421), inasmuch as one of the mutants was found to have been partially sterile. Metz and Bridges did adduce another example in *D. ampelophila*, but for a variety of reasons that, even if substantiated, would scarcely be to the point. As a matter of fact, however, in so far as opportunity of repeating the cross has occurred, complete fertility resulted. I know of no other example to which Dr. Cunningham can be referring.

Mr. Crowther (NATURE, June 17, p. 777) mistakes my meaning. It is, as he says, not difficult to "imagine" interspecific sterility produced by a gradual (or sudden) modification. That sterility may quite reasonably be supposed to be due to the inability of certain chromosomes to conjugate, and Mr. Crowther's simile of the sword and the scabbard may serve to depict the sort of thing we might expect to happen. But the difficulty is that we have never seen it happen to swords and scabbards which we know to have belonged originally to each other. On the contrary, they seem always to fit each other, whatever diversities they may have acquired.

W. BATESON.

July 2, 1922.

Geology and the Nebular Theory.

I DO not deserve the reproaches of Prof. Coleman (NATURE, June 17, p. 775). My molten earth did not, in point of fact, owe its thermal energy to primitive condensation, but to accumulated radioactive heat, as the concluding part of my lecture might have shown. I am committed neither to the nebular theory nor to the planetesimal theory.

Had I adopted the wider definition of the Archæan favoured by Prof. Coleman, I could not, of course, have described the Archæan sediments as scanty. I referred to an Archæan limited to the Keewatin, and to the Laurentian outpourings of granitic materials. The Keewatin is generally described as mainly volcanic in origin. The definition of Archæan and Algonkian favoured by Van Hise and Leith (Bulletin 360 of the United States Geol. Survey) would bear me out.

While many geologists would agree with Prof. Coleman as to his estimates of Archæan sediments, many, I think, will disagree with him in his contention that there was nothing exceptional in the thermal conditions attending the Laurentian revolution. Prof. Coleman's most interesting discovery of an ice age in Huronian times has, I submit, nothing to do with the matter. On the other hand, I think geologists in their interpretation of the Archæan should keep in mind the possibility (or probability) that the phenomena observed are due to paroxysmal thermal developments traceable to deep-seated radioactive substances: and that these developments, which appear to have been world-wide in extension, may have been sufficiently intense to have closed a biological era. So that, in fact, we have in the Archæan the almost obliterated record of a prior geological age.

J. JOLY.

Trinity College, Dublin.

I HAVE read with interest Prof. Coleman's timely reminder, in NATURE of June 17, p. 775, of the essentially intrusive relations of the Archæan and of the frequently made deduction that the oldest visible rocks of the earth's surface are sedimentary. Of course this deduction is perfectly sound, provided the age of an intrusive rock is taken, as has been the

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custom, to be that of its intrusion. We are entitled, however, to consider the previous history of the material composing these intrusive Archæan masses, and, in view of their predominantly banded structure, which marks them off as in some way different from later intrusive masses of similar composition, such consideration seems forced upon us.

Now the banded character of the Archæan gneiss suggests a partial derivation by melting from some stratiform materials such as sedimentary or volcanic rocks, or at any rate from rocks showing marked small-scale differentiation into basic and acid types. I do not think that stratiform differentiation during or previous to crystallisation can be seriously put forward as a cause of the banding, in view of the rarity of this phenomenon in more recent granites, and the fact that in them it is largely a marginal effect.

May we not then have in these Archæan gneisses the recrystallised remnants of still older sediments and lavas, and who is to say that they may not also embrace portions of the original surface on which water first settled, but so obscured by recrystallisation that the question of its molten or planetesimal origin is now unsolvable ?

The difference between the two views is simply that one regards the history of sedimentation on the earth as cut off sharply by intrusion, while the other sees it extending still further back into the mists of the past, beyond the point where human vision is any longer capable of discrimination. Where, on either view, is the decisive criterion between the nebular and planetesimal hypotheses ?

W. B. WRIGHT.

Manchester, June 27, 1922.

Wegener's Displacement Theory.

WEGENER'S speculations have attracted so much attention that there must be many who would be glad to find some simple means of testing his fittings and coincidences for themselves. Owing to the distortion present in all maps such tests must be carried out on a globe. Wegener himself uses tracing paper, which must be cut and slashed in order that it may even approximately fit the surface ; and any one who has tried it will admit that it is difficult to obtain satisfactory results. An easier plan is to roll out a lump of modelling wax or plasticine into a sheet of moderate thickness. The sheet may then be pressed upon the globe and cut to the required shape. According to my own experience, the best method is to cut the sheet a little smaller than the area that is to be represented, so that the actual margin appears all round it, and to build it outwards to this margin by the addition of small pieces of wax. Old plasticine which has become rather dry works very well and does not stick to the globe.

But much more precise tests can be carried out with the help of some form of triangular compasses. The three points of the compasses may be placed on three critical points of the globe and afterwards transferred, without altering their relative positions, to any other part of the globe that may be desired. The ordinary triangular compasses of the draughtsman are very little use upon a spherical surface, but a fairly convenient instrument can be constructed with an ordinary one-jointed two-foot rule as its basis. A point about an inch long is fixed near the joint, and each arm is provided with a sliding carrier. Each carrier bears a short sleeve through which a pointed rod, such as a knitting needle, slides rather stiffly. These rods form the other two points, and all three should stand approximately at right angles to the plane of the rule.

This is an easily constructed type, but much more convenient forms can be devised. If, for example, the arms are arcs of circles, of suitable diameter, so that they may stand concentric with the globe, the points may all be of fixed length, and the most troublesome of the adjustments required by the straight-armed form will be avoided.

This is not the place to discuss Wegener's views, but the use of triangular compasses seems to show that a rather high degree of plasticity is necessary in the masses of "Sial" in order to produce the coincidences on which he bases his calculation of the probability that his theory is correct.

PHILIP LAKE.

Sedgwick Museum,
Cambridge, June 21.

Opalescence Phenomena in Liquid Mixtures.

IT is well known that liquids which mix completely above a certain critical temperature, *e.g.* phenol and water, exhibit a strong and characteristic opalescence as the temperature of the mixture is lowered to a point slightly above that at which the components separate. A quantitative theory of this phenomenon was put forward by Einstein (*Annalen der Physik*, vol. 33, 1910) on the basis of thermodynamical reasoning, the spontaneous local fluctuations of *concentration* of the mixture being taken into account and the light-scattering due to the resulting fluctuations of refractive index being evaluated. He obtained as the expression for the light-scattering

$$\pi^2(\mathrm{M}/\mathrm{N}\lambda^4)\,.\,v\left(\frac{\partial\mu^2}{\partial\kappa}\right)^2\Big/\frac{\partial\,(\log p)}{\partial\kappa}\ \text{per unit volume,}$$

where μ is the refractive index of the mixture and $\partial(\log p)/\partial\kappa$ expresses the rate of change of the vapour pressure of one of the components with concentration, a quantity which becomes very large as the critical temperature and concentration are approached, thus giving rise to a marked opalescence. It should be pointed out, however, that Einstein's expression does not include the whole effect, for we have also to consider the result of *the fluctuation of density of either component taken separately*, and to add to Einstein's formula

$$(\pi^2/18)(\mathrm{RT}/\mathrm{N}\lambda^4)[\beta_1({\mu_1}^2-1)^2({\mu_1}^2+2)^2 \\ +\beta_2({\mu_2}^2-1)^2({\mu_2}^2+2)^2],$$

where β_1, β_2, μ_1, μ_2 are respectively the compressibilities and refractive indices of the components. Further, the light-scattering due to the anisotropy and arbitrary orientation of the molecules of the components has also to be added.

The result of these corrections of Einstein's investigation may briefly be indicated. Very near the temperature at which the mixture separates into two phases, the fluctuations of *concentration* contribute by far the larger portion of the effect. But at higher and lower temperatures the effects of fluctuations of *density* and molecular anisotropy are no longer negligible, and when the temperature is sufficiently removed from the critical point they form a substantial part of the whole. Further, the increase in relative importance of the effect of molecular anisotropy in these circumstances should result in an increase in the proportion of *unpolarised* light in the transversely-scattered beam as we recede from the critical temperature.

The foregoing indications of theory have been confirmed generally in a series of experiments over a wide range of temperatures on light-scattering in phenol-water mixtures undertaken under the writer's direction by Mr. V. S. Tamma. It is found that the increased opalescence of the mixture over

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